HCI Research as Problem-Solving

The authors of the paper - "HCI research as problem solving" have discussed the five aspects of problem solving capacity which are significance, effectiveness, efficiency, transfer, and confidence. However, they haven't addressed security as a problem pertaining to HCI systems.

There is a major concern with respect to security of devices like Smart Home(Nest), Smart Lights for home etc. where hackers can easily get access to such devices and misuse them. The paper – “Scaling the Security Wall: Developing a Security Behavior Intentions Scale (SeBIS)”[12] that has been cited, performs analysis on measures that go towards selection of passwords, proactive awareness and other factors. But, why haven’t they considered it as a criterion to solve reliability of systems?

-Sireesha

1. Author's discuss how the best CHI papers focus on empirical and constructive problems rather than conceptual problems. The overall aim of this paper is to focus on how to improve HCI research overall, so I do not see how focusing conceptual problems rather than empirical or constructive problems will improve the HCI research.

Empirical problem has been defined as: Empirical research is aimed at *creating or elaborating descriptions of real-world phenomena related to human use of computing*.

Constructive problem has been defined as: Constructive research is aimed at *producing understanding about the construction of an interactive artefact for some purpose in human use of computing*.

Looking at the current scenarios and development in technology, solving the above mentioned problems should be of prime importance.

For example: Using Interactive Voice Response (IVR) - in Sangeet Swara - to curate high-quality response for playback to its user, seems much more practical than solving the conceptual problem of 'what makes an interruption disruptive?'. Sangeet Swara seems like a practical implementation of HCI which solves an existing and practical problem. By reading the gist of the two papers, first one seems to have provided more in terms of effectiveness than the latter.

2. Authors mention one of the limitations as - 'HCI problems tend to be messy. How does problem-solving fare with ill-defined or ‘wicked’ problems?'

This is one of the major points that should be focused on. Having a good problem-solving approach is great, given the problem is not complex and well-defined. HCI problems are complex. Before having a problem-solving approach one needs to focus on making the problem simple and well-defined.

Once the problem is non-complex and easy to understand, providing a robust and effective solution will become easier. For example: divide-and-conquer approach to segregate a problem into a bunch of simpler problems and focusing on one problem at a time.

-Debkanya

I never really thought about research as not being about problem-solving. I am new to the world of HCI, but in other academic/research papers it has always seemed to be about problem solving.There is some issue that needs a solution or a better solution. Perhaps I misunderstand what they are getting at, but why do research on a topic if it is already at its best? Which I will argue that everything can use improvement.

The authors add a fifth aspect of problem-solving capacity of confidence - page 4959. Of the research papers I have read, it seems that so many are only producing these results in controlled environments. It has always frustrated me; I understand that it can be a starting point for the topic. For example, I read many papers about making programs concurrent. Even today, it is still a relatively new field, but we were told to read papers written 10 or more years ago. Technology changed significantly making that particular paper not the most relevant. In regards to HCI, I feel a lot of the design concepts can be continued for a much longer time frame - color choices, fonts, radio buttons vs drop down lists, etc. Perhaps newer technology such as touch screens or voice control may affect these "standards." Confidence is definitely something that should be considered in all research.

-Karen

In this recent paper both Oulasvirta and Hornbaek talk extensively about HCI being an enigma, so much so, that a couple of paragraphs into the paper we are told "*that HCI has been criticized for lack of 'motor themes, mainstream topics and school of thought 'and for being fragmented 'across topics, theories, methods, and people'.* " The second part of this line resonated with me in particular. Last year, as I was reading Schutt and O'Neil's 'Doing Data Science', chapter one in that book battles with a similar question of defining an entity that is fairly new and establishing the class as a 'science'.

I also found parallels between Drew Conway's Venn diagram of data science and Oulasvirta and Hornbaek's 'Three Types of Research in HCI'. Math and Statistics Knowledge finds parallels with Empirical problems. Empirical problems are also referred to as direct problems that Qualitative research (ethnography) can solve. Hacking Skills finds parallels in Conceptual Problems or indirect/second-order problems. And finally, Substantive Expertise finds parallels with Constructive problems (both put immense emphasis on 'understanding'. This personal revelation left me confused; do we really need to define a field (such as HCI or data science) with the best possible articulation? Isn't the foundation for most fields in the sciences pretty much the same?

The five aspects of Problem-solving capacity also got me thinking; especially, *transfer* (how well a solution transfers to instances of the same problem). The first example that came to my mind was Uber. As an on-demand taxi service, it gained traction quite quickly in the USA. However, the same UI led to a completely different user experience in India. Ambient reasons like government regulations aside, the company needed a deeper ethnographic understanding before they could start gaining a user base that was on par with the one they had in the States. Similarly, many products that are highly successful in countries like India, fail to do well in the West (Examples: Zomato/Urbanpoon or Blue Apron/Halfteaspoon). This is also why, I completely endorse the authors adding a new aspect, *confidence*. I believe directly transferring a solution that worked for one problem may not always work for another problem with the same

exact instances. Some papers addressed *confidence* by conducting user studies with a large sample and doing a follow-up later.

It was also fascinating to read some of the authors inferences, one that particularly stood out was about how half of CHI Best papers were focused on solving practical problems. This goes back to the first two type of HCI research problems (empirical and Conceptual). It also got me thinking that perhaps, the need to solve everyday problems is inherent and deep-rooted in all of us. For example, the current class of CS 522 has quite a few people who did not take 422 (myself-included) or any *viz* based class in the past, yet most of us came up with ideas that solve a problem most of us faced at some point or another - be it a burnt pan, or the feeling of being unsafe around campus.

-Vishnu